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Mireles-Flores, Luis

2018

Mireles-Flores , L 2018 , ' Recent trends in economic methodology : a literature review ' , Research in the History of Economic Thought and Methodology , vol. 36 , no. A , pp. 93-126 . https://doi.org/10.1108/S0743-41542018000036A008

http://hdl.handle.net/10138/305681
https://doi.org/10.1108/S0743-41542018000036A008

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RECENT TRENDS IN ECONOMIC METHODOLOGY: A LITERATURE REVIEW

Luis Mireles-Flores

ABSTRACT

This essay is a review of the recent literature on the methodology of economics, with a focus on three broad trends that have defined the core lines of research within the discipline during the last two decades. These trends are: (a) the philosophical analysis of economic modelling and economic explanation; (b) the epistemology of causal inference, evidence diversity and evidence-based policy and (c) the investigation of the methodological underpinnings and public policy implications of behavioural economics. The final output is inevitably not exhaustive, yet it aims at offering a fair taste of some of the most representative questions in the field on which many philosophers, methodologists and social scientists have recently been placing a great deal of intellectual effort. The topics and references compiled in this review should serve at least as safe introductions to some of the central research questions in the philosophy and methodology of economics.

Keywords: Modelling; explanation; causality; evidence; economic policy; behavioural economics

Research in the History of Economic Thought and Methodology: Including a Symposium on Bruce Caldwell's *Beyond Positivism* after 35 Years
Research in the History of Economic Thought and Methodology, Volume 36A, 93–126

It is almost 40 years since modern texts explicitly devoted to the philosophy and methodology of economics began to appear. Many changes in the topics of research have occurred during this short period, some of them related to direct changes in economic theory and practice, while others have been by-products of recent developments in the philosophy of science (see Hands, 2015a). Four decades ago, only a handful of economists and philosophers of science were interested in discussing epistemological issues related to economics. Today, journals and institutes specialised in research combining philosophy and economics have emerged and thrived. Moreover, a continuously growing number of graduate students trained in philosophy, economics, history, sociology, political science, statistics, and so on, now explicitly aim at engaging with the wide range of socially relevant issues studied under the label of philosophy of economics.

This essay is an attempt to showcase some of the most significant current topics of research in the methodology and philosophy of economics. Given the constantly increasing amount of contributions to the discipline, several topics and many recent high-quality and innovative pieces of research unfortunately had to be left out. So, this is a patently non-exhaustive and selective literature review. Here I offer a brief description of the selection criteria.

I have grouped the topics into three broad trends of research: (1) modelling and explanation in economics, (2) causality and evidence in economics and (3) issues concerning behavioural economics. Obviously, there are other trends going on, yet these are three of the most commonly and extensively researched areas in current economic methodology. I have usually followed the chronological development of the accounts and debates to guide the narrative of the story. Although, admittedly, in many cases there is no proper story yet to be told, but only rather intriguing open questions.

In general, I have included mostly works published during the last 15 years, yet I made a number of exceptions by including a few older books or articles that are essential to properly understand the origins of certain topics of discussion. My hope is that mentioning a few foundational texts connected to current debates will be useful to readers who are not yet very familiar with philosophy of economics.

In relation to the specific contents and details of the material, I aimed at including the most representative pieces on each question or debate, and also those which offer good summaries of the specialised literature on their respective topics. I tried to avoid including 'purely' philosophical contributions or pieces which merely use economic examples or economic ideas to illustrate highly abstract philosophical points. Instead I focused on literature that engages with epistemological issues that are characteristic in economics or that have implications for scientific and policy-oriented economic practice.

Finally, my own familiarity with the relevant literature and my philosophical interests are unavoidably reflected in the final selection of the reviewed material. Nonetheless, even if there could be reservations about the merits of my

selection criteria, I genuinely believe that all the manuscripts I have included are exemplary of the excellent academic standards currently ruling in the field, and that all of them will be highly fruitful and rewarding readings to any philosophically minded social researcher.

MODELLING AND EXPLANATION IN ECONOMICS

One of the concerns that has motivated the most philosophical reflection and controversy about modelling in economics seems to be the wish to come to terms with falsehood. J. S. Mill's (1844 [1830]) methodological appraisal of economics is already, to a great extent, an attempt to justify how false and abstract conceptualisations of economic agents and phenomena can possibly be the source of reliable scientific knowledge. The same aspiration, namely to offer a justification of how economics can be epistemically valuable in spite of being theoretically founded on false propositions (idealisations, abstractions, analogies, conjectures and the like) is clearly behind other classical methodological elucidations, such as Friedman's (1953), Gibbard and Varian's (1978), or Musgrave's (1981). How can models constructed on unrealistic foundations be of any use to explaining, understanding or predicting real economic phenomena?

Philosophical accounts about modelling, explanation, understanding, truthfulness and realisticness in economics have certainly proliferated in the last decades. Of course, the question about how false models can lead to reliable scientific knowledge is not a concern exclusive to economics. Most philosophers of science seem to agree that, for better or worse, there is no meaningful way to come up with the perfect 'picture' of reality by using our subjective scientific constructs (see, e.g. Cartwright, 1983; Dupré 1993; Teller, 2001; van Fraassen, 1980). The only special aspect about modelling in economics is, perhaps, that in contrast to all other sciences, theoretical economic results are often shamelessly presented and celebrated as if there was nothing more to the practice of economics apart from cultivating and elevating the craft of formal modelling (see Colander, 2010; Hodgson, 2009; Pfleiderer, 2014; Romer, 2015).

The recent philosophical research on economic modelling has been directly influenced by the literature on scientific representation in the philosophy of science (some very influential pieces are, e.g. Giere, 2006; Suppes, 2002; van Fraassen, 2008; Weisberg, 2013; Wimsatt, 2007). Well-known general accounts that sparked initial debate within economic methodology are Hausman's (1992), and Mäki's (1992, 1994, 2004) characterisations of economics and modelling. Both accounts were, if not influenced, at least very much in line with a perspectivist view of scientific modelling, like that hoisted by Giere (1990, 2004, 2006). Agents use models to represent some aspects of economic phenomena to some relevant extent, for particular epistemic purposes.

Mäki has argued that a model's main epistemic function is to isolate relevant aspects of the phenomena under study by means of idealisations and abstractions. His account of modelling as isolation has continued developing throughout the years and has evolved in content and sophistication. In Mäki's most recent account, Giere's four elements of representation, namely 'agents', 'models', 'targets' and 'purposes', are supplemented by 'audiences' to whom the model is intended to serve, and 'commentaries' whereby modellers address all sorts of issues and clarifications about the intended resemblance and representativeness of the model (see Mäki, 2009, 2011). But perhaps not all economics can be encompassed by the practice of isolating. As a counterexample to Mäki's characterisation of modelling in economics, Alexandrova (2006) has suggested that the case of the actual design of the Federal Communications Commission (FCC) telecommunication auctions constitutes an instance of economics being successfully applied without using the method of isolation.

Knuuttila (2009) has compared two alternative approaches to economic modelling, the 'isolationist view' and what she calls the 'credible constructions' approach. The former approach is meant to isolate causal relations and mechanisms that are supposed to be at work in reality (e.g. Mäki, 1992, 2004), the latter is meant to build 'hypothetical' models from which potential credible inferences can be drawn (e.g. Sugden, 2000). She suggests that the constructionist perspective can better accommodate the way scientists actually learn when using models. Also, Grüne-Yanoff (2011) has questioned Mäki's isolationist account of economic modelling. After pondering some counterexamples, he concludes that model building cannot be exhaustively described by the method of isolation, and that the outcomes of models are also not isolations. Morgan (2012) provides a full-fledged (historical and methodological) account about the building of models, using different modelling strategies (which include imagining, caricaturising, idealising and so forth), and she elaborates on how economists have used them in a variety of ways to reason and to make inferences (see also Morgan, 2015; Morgan & Knuuttila, 2012).

Recently, the very idea that a theory of representation is necessary to understand scientific modelling has been put into question in the philosophy of science (see Morrison, 2015). Yet, the discussion of models in economics has for the most part taken for granted that models are epistemically valuable in virtue of being 'representational' tools. Grüne-Yanoff (2009, 2013) has challenged this generalised idea, and characterised so-called 'minimal models' as theoretical models which contribute in different valuable ways to how scientists learn about real-world phenomena (see also Claveau & Vergara Fernández, 2015; Thoma, 2016), even if such models do not hold any traditional representational relation to the world. Indeed, many theoretical models could be characterised as minimal models in the sense just described. However, Fumagalli (2016a) has recently argued against the possibility of actually learning anything from such minimal models.

Modelling Assumptions and Robustness Analysis

Another closely related traditional topic of debate in relation to economic models is the realisticness (or the lack thereof) of assumptions (e.g. Cartwright, 2006a; Hindriks, 2006; Mäki, 2000). False assumptions in models are not problematic, as long as they are negligible or irrelevant to the model's main results (Mäki, 2006). Assumptions that have a direct effect or are responsible for the model's result are labelled as 'substantial'. Only substantial assumptions are the ones carrying some truth about the relevant target phenomena. The straightforward questions to ask then are: how can economists tell which assumptions are substantial and which are not? And whenever economic models fail or are shown to be wrong, how do economists know which assumptions are to be blamed and discarded or revised?

In a relatively recent article, Kuorikoski, Lehtinen, and Marchionni (2010) noticed that most of economic theorising consists in building models with slightly different assumptions, and yet generate the same theoretical result. Inspired by Wimsatt's (2007) ideas on robustness, they argue that such type of theoretical modelling is a form of derivational robustness analysis whereby economists attempt to test which assumptions are substantial and which are not. Again, substantial assumptions are those which make a difference to the theoretical result when they are changed or relaxed. Thus, there is some epistemic gain in theoretical modelling by simply allowing economists to tell apart the relevant assumptions from those that are irrelevant to the derived results.

As a reaction, Odenbaugh and Alexandrova (2011) have pointed out that robustness analysis provides economists with a tool for discovery, yet not for empirical confirmation. Theoretical models are open templates (or 'open formulae') which can be used to build hypotheses about the mechanisms at work in real-life phenomena. Derivational robustness analysis at best offers economists a way to discriminate among the wide array of different explanatory templates in relation to a particular target (see also Alexandrova, 2008; Kuorikoski, Lehtinen, & Marchionni, 2012). Houkes and Vaesen (2012) also contest that robustness analysis can have confirmatory power and identify two alternative roles for it.

In contrast, Lehtinen (2016, 2017) argues that there is in fact some confirmatory import in robustness analysis. In some cases, he argues, the validity of a theoretical result becomes more solid as a consequence of what he calls 'indirect confirmation'. Broadly put, the idea is that an assumption $\mathbf{A_1}$ in one model can receive some indirect confirmation after the result of a different model (but one in which $\mathbf{A_1}$ also features) is empirically confirmed.

This debate on the epistemic virtues of robustness analysis opened up a number of interesting questions about the ways in which modelling practices actually contribute to science. How can one be sure that derivational robustness is not leading modellers (unknowingly) to be more prone to confirm one result

over another? Can derivational robustness analysis really be a method of confirmation? What are the implications for economic theorising? Are economists really engaging in such analysis when they build models, or is robustness analysis only an *as-if* characterisation of what they do? Should assumptions be tested against other assumptions or against empirical facts?

Derivational robustness analysis has been mostly discussed as an aspect of scientific modelling in general, so recent philosophical contributions to the issue (which are highly relevant to understanding the implications of robustness for economics) have mainly been published in general philosophy of science venues (see, e.g. Forber, 2010; Lloyd, 2015; Parker, 2011; Raerinne, 2013; Weisberg, 2013). For a comprehensive analysis of the problem of robustness in science, in general, see the edited volume by Soler, Trizio, Nickles, and Wimsatt (2012).

In a very recent contribution, Lisciandra (2017) clarifies different connotations of derivational robustness and points to some problems that such analysis might have when scientists are unable to only slightly alter the models that are to be compared. Using examples from biology and economics, she puts forward that the problem of how to properly compare the results from structurally different models remains open to further philosophical investigation.

Modelling and Explanation

Questions about whether and how models can contribute to scientific explanations have been discussed for a long time in the philosophy of science (for recent relevant treatments, see Batterman & Rice, 2014; Bokulich, 2011; Kennedy, 2012; Kuorikoski & Ylikoski, 2015; Rice, 2015). Standard accounts of explanation suggest that successful explanations increase in one way or another our understanding of the phenomena to be explained (see, e.g. de Regt, 2009; Hindriks, 2013; Strevens, 2013; Trout, 2007). Given the longstanding worry about the falsehood that is inherent in economic models, the existing philosophical inquiries about explanation are especially relevant for economic methodologists. How can economic models 'explain' or 'be useful to providing explanations' when they contain mostly blatantly false assumptions?

Some recent attempts to account for the explanatory import of economic models are Mäki's (2009, 2011, 2017) account of models as credible surrogate systems (already mentioned above), and Sugden's (2000, 2009, 2011) view of theoretical economic models as useful 'credible worlds'. Somewhat counterintuitively, but perhaps very much in accordance with what economic theorists actually try to do, Sugden (2011) argues that there is some epistemic value in creating theoretical 'credible model worlds' to study regularities that operate within those theoretical constructs, even before finding any regularities in the real world that the models could potentially help explaining.

In contrast, Alexandrova (2008) has given an account of models as templates ('open formulae'), which totally rejects the claim that models are explanatory (see also Alexandrova & Northcott, 2009, 2013). Rather, models play a heuristic role in the conjecture of causal hypotheses, which would then still require further empirical confirmation in order to establish theoretical results as scientific knowledge.

The debates about the explanatory power of models in economics became fairly noticeable in the methodological arena after the publication of an article by Reiss (2012) titled 'The explanation paradox'. First, Reiss discusses (drawing from Wimsatt, 2007) different types of 'false' idealisations as they are typically employed in economic modelling, and observes that in spite of including falsehood, economic models are thought to be explanatory. Then he proposes what he calls a paradox as follows (Reiss, 2012, p. 49): (1) Economic models are false. (2) Economic models are nevertheless explanatory. (3) Only true accounts can explain.

The rest of the article is an appraisal of the different attempts that have been made to deal with or to get rid of each of these premises (trying to avoid the paradox) in the literature on economic modelling. Reiss ponders and finds lacking existing accounts of how models with false assumptions can be truthful (e.g. Mäki, 2009, 2011), about how models need not be explanatory (e. g. Alexandrova, 2008), and about how it can be possible to explain real world phenomena from theoretical constructs that are not truthful (e.g. Sugden, 2011). His conclusion is that the paradox is genuine and that, for the moment, the only response to it 'is to remain baffled' (Reiss, 2012, p. 59).

Independently of accepting its main proposal or not, Reiss's article is a great critical exposition of some of the most influential recent ideas about economic modelling. Furthermore, it has kindled more debate among methodologists on each of the issues that the alleged paradox highlights. The first reactions to Reiss's article by Alexandrova, Northcott, Grüne-Yanoff, Hausman, Mäki, Rol and Sugden – followed by Reiss's response – can be found in a special issue of the *Journal of Economic Methodology* (vol. 20, issue 3, 2013).

There are also some recent proposals suggesting that the epistemic import of economic models consists in providing some form of *how-possibly* explanations, in contrast to *how-actually* explanations (e.g. Aydınonat, 2007; Grüne-Yanoff, 2009, 2013; Hands, 2016; Rohwer & Rice, 2013). How-possibly explanations need not be true about the explanandum, but only hypothetical and credible under certain conditions. This would justify, for instance, the existence, and high appreciation, of some theoretical models which are extremely formal and obviously not meant to provide actual explanations of anything. According to Verreault-Julien (2017), there can even be cases of economic models which can provide understanding, even when they provide neither actual nor possible explanations. Purely mathematical models, he argues, such as the Arrow–Debreu model, provide mathematical how-possibly explanations

which, in turn, increase our understanding by establishing claims of mathematical dependence.

Methodological discussions, however, have tended to assess the potential epistemic virtues of economic models by studying one type or one particular 'famous' model in isolation. As a recent proposal, Ylikoski and Aydınonat (2014) suggest that in actual scientific practice, models are typically developed, modified, and refined in clusters that share a 'common core' of assumptions. Understanding the epistemic virtues of modelling, then, requires understanding how families of models evolve.

There is very little philosophical research about how clusters of models are used to generate explanations, especially of economic phenomena. It follows from Ylikoski and Aydınonat's account that the answer to 'how do economists explain?' is that they employ clusters of multiple models, rather than a single one, to construct explanations of economic phenomena. Interestingly, this account is in various essential respects closer to what economic practitioners actually believe to be the essence of economic modelling (see, e.g. Aydınonat, 2017; Rodrik, 2015).

CAUSAL INFERENCE AND EVIDENCE IN ECONOMICS

As is well known, Hume and J. S. Mill were two of the first philosopher-economists to display a deep interest in the nature and epistemology of causation. Knowledge about causal relations is valuable for explanatory and instrumental purposes. Understanding the causes of phenomena gives us predictive and controlling powers. In Mill's words, 'of all truths relating to phenomena' knowledge about causation is 'the most valuable to us', because we base on it 'every reasonable anticipation of future facts, and whatever power we possess of influencing those facts to our advantage' (Mill, 1874 [1843], 3.5.1).

During the 20th century, philosophers interested in causality typically focused their attention on related, but fundamentally distinct types of questions, including conceptual and semantic questions such as 'what do "cause" and "causing" mean?' (e.g. Ducasse, 1926; Lewis, 1973; Russell, 1912–1913) or 'what is the logical form of a causal claim?' (e.g. Davidson, 1967); ontological questions like 'is there causation in the real world?' and 'what is the nature of causation?' (e.g. Dowe, 2000; Menzies, 1989; Salmon, 1980) and epistemological questions such as 'how can one distinguish, find, and learn about causal relations?' (e.g. Pearl, 2000; Simon, 1954; Spirtes, Glymour, & Scheines, 1993; Suppes, 1970).

The mysterious and elusive ontological nature of causation has for a long time made economists shun causal talk. In general, the metaphysics of causation remains an unsettled issue and a persistent topic of debate among philosophers up until the present day (see, e.g. Beebee, Hitchcock, & Menzies, 2009;

Price & Corry, 2007). Nevertheless, as univocal and reductionist projects about the nature of causality began to fade away at the turn of the 20th century, and different pluralistic approaches began to emerge (see Campaner & Galavotti, 2007; Cartwright, 2004; Hitchcock, 2003; Psillos, 2009), the philosophical focus of investigation also moved from ontological worries towards projects concerned with developing and improving ways to reliably find causal relations, that is methods of causal inference.

It is this latter epistemological angle about causation which has gradually and increasingly been reintroduced into economics and other social sciences during the last decades (see Heckman, 2000; Hoover, 2001; Morgan, 2013). Reiss's (2015) recent monograph is a neat introduction to the different theories of causality, to causal pluralism, and to the philosophical relevance of causal inference to the social sciences. Nowadays, causal notions and methods of causal inference are openly endorsed, used and debated among economists in, for instance, econometrics and in economic policy analysis.

Econometrics with or without Theory

Some of the current methodological issues related to causal inference in economics follow from the basic motivations behind traditional approaches to econometrics. Different views about the theoretical foundations of econometric models have led to significant methodological debates (for some philosophically informed expositions of the historical and methodological development of econometrics, see Hendry & Morgan 1995; Hoover, 2012a, 2015; Morgan, 1990, 2001).

The Cowles Commission approach – mainly based on Haavelmo's (1944) ideas – is a theory-based approach. The econometric analysis following this approach presupposes causal connections which are taken from economic theory, represented in a system of equations, and then identifies and measures their strength by means of statistical tools. If the errors in the system follow definite probability distributions, and are uncorrelated with each other, then it is possible to infer parameters for these equations. This is, of course, a very big 'if', and indeed structural modelling of this kind has had serious problems trying to get the real values of parameters, mainly because of the difficulties in testing the validity of a priori assumptions (on different assumptions imposed to error terms and their distinct aims, see Fennell, 2011).

Sims (1980) proposed instead a non-structural characterisation of macroeconometric models called 'vector autoregression' (VAR). In this approach, every variable is set to be dependent on its own lagged values and on those of the other variables in the system. The starting presupposition is that everything could depend on everything, and so the approach has the apparent advantage of not presupposing any background causal knowledge. However, the system is

left unidentified, since the dependence relations between the endogenous variables only reflect correlations. The lack of theoretical presuppositions precludes the possibility of making counterfactual inferences, which are required, for instance, to make policy inferences (see Reiss & Cartwright, 2004).

At least some structure was needed to reliably tell causal relations from mere correlations. Consequently, structural restrictions were added on linear transformations of the VARs, which resulted in the so-called 'structural vector autoregressions' (SVARs). By imposing the restrictive assumptions, which vary from model to model, SVARs allow the analysis of structural relations between contemporaneous variables, and thus they allow a causal identification of the system (see Hoover, 2005 for more details and contrasts between VARs and SVARs). Still, the required a priori assumptions introduce again some potential theoretical bias into the analysis.

The overall issue of interest to philosophers and methodologists can be very broadly put then as follows: there is a trade-off between including preconceived theoretical assumptions in econometric analysis and the validity of the causal inferences that can be made from it. Causal interpretation of econometric models requires background knowledge about the correct specification of the equations. On the one hand, too much a priori theory providing the system with structure yields questions about whether the inferred coefficients actually stand for real parameters. On the other hand, not enough a priori theory about the causal structure, the properties of the error terms, and so on, precludes any meaningful way to make reliable counterfactual inferences from the analysis. This trade-off has been a topic of methodological debate among applied economists and econometricians for many years (see, e.g. Hendry, 1980; Hoover, 2013; Hoover & Perez, 2004; Leamer, 1983; Pesarana & Smith, 1995; Wolpin, 2013).

In connection to this debate in econometrics, but also as a contribution to the available philosophical accounts, Hoover (2001) has put forward an interventionist account of causality (analogous, but not identical to Pearl's, 2000; or to Woodward's, 2003). Hoover's proposal focuses on causal structures and on the inference of causal direction between macroeconomic variables. Given different alternative causal interpretations of systems of equations, for example, one positing that X causes Y and another that Y causes X, and assuming that at least one system reflects the true (but unknown) causal order, Hoover's method consists in a comparison of the relevant parameters in the systems after the identification of a structural break (an exogenous and localised intervention). The stability of the parameters in one system after the structural intervention, in contrast to the instability of those in the alternative system, allows the identification of the right causal direction. To illustrate the proposed structural test, Hoover uses macroeconomic data to infer the causal direction between taxes and government spending, and between money and prices (Hoover, 2001, Chapters 8 and 9). An innovative feature of this project is that it openly takes a perspectivist approach to causal structure, in which, rather than avoiding the

use of theoretical assumptions, they are simply made explicit and assessed relative to their purpose. More recent elaborations of Hoover's structural approach are expressly connected to pragmatism and perspectival realism (see, e.g. Hoover, 2012b, 2012c).

Another philosophically rich approach to econometrics has been put forward by Aris Spanos. In a recent article, Spanos (2012) analyses in detail several philosophical/methodological issues in current conventional (textbook) econometrics. According to Spanos, a large amount of inferences made in common econometric practice are not justified, because the majority of researchers are content with poor methodological reflection about the statistical presuppositions in their models. In particular, he stresses the reticence to properly test the validity of statistical assumptions about model specification. He characterises the problem as a form of dogmatism of what he calls the pre-eminence of theory (PET) perspective, which is said to be 'the methodological framework that has dominated empirical modelling in economics since Ricardo' (Spanos, 2012, p. 334). Spanos's alternative is the so-called 'error-statistical approach to empirical induction', which he has developed and defended for several years in collaboration with Mayo (see, e.g. Mayo & Spanos, 2010). The error-statistical view, he argues, can help dealing with most of the methodological problems of the PET perspective in econometrics (see also Spanos, 1986, 2008, 2010, 2015).

Potential Outcomes and the Design-based Approach

As another reaction to the theory-based approaches to causal inference, some researchers have turned to design-based econometric analysis. The idea is to minimise the use of implicit theory to test causal relations by using techniques similar to those employed in controlled experimental designs. In contrast to theory-based econometricians, who aim at using structural models to make counterfactual policy predictions, design-based researchers focus on the causal interpretation of data to evaluate the outcomes of already existing policy implementations.

The basic rationale of this approach has been known among statisticians as the 'potential outcomes framework' (see Holland, 1986; Rubin, 1990, 2005). According to Morgan and Winship (2007), the logic behind the potential outcomes can be captured by the 'counterfactual model for data analysis'. Broadly put, given that there are some well-defined causal states X: x_0 and x_1, for the members of a population P, for each unit u_i, there are two outcome values Y: y_1 for the outcome of the 'treatment' state x_1, and y_0 for the outcome of the 'control' state x_0. One of these outcomes is observed for each u_i, while the other is hypothetical or 'potential'. The causal effect on Y for a single unit is then defined as the arithmetic difference between the two values $(y_1 - y_0)$. While at the individual level, both values y_1 and y_0 cannot be simultaneously observed in

the same unit, at the aggregate level the expected values $E(y_1)$ and $E(y_0)$ can be calculated from observed available data sampled from the relevant population, and hence it is possible to infer an average causal effect (ACE) $= E(y_1 - y_0) = E(y_1) - E(y_0)$ for the whole population (see Morgan & Winship, 2007, Chapter 2).

Holland (1986) provides a classical description of the framework and discusses explicit connections to, and contrasts with, traditional causal theories in philosophy. More recently, the quasi-experimental approach has been advocated as a 'mostly harmless econometrics' (Angrist & Pischke, 2009), in opposition to 'harmful' theory-based approaches, mainly because it avoids the *implicit* reliance on a priori theoretical assumptions to isolate and estimate causal effects. Instead, the design-based approach searches for databases that can be interpreted and analysed as if the data had been generated by a randomisation procedure, while any methodological assumption required for the causal inference is, at least in principle, made as explicit as possible.

The approach has become rather popular among empirical economists in areas such as development and growth economics (see, e.g. Banerjee, 2007; Banerjee & Duflo, 2011; Cohen & Easterly, 2009; Söderbom, Teal, Eberhardt, Quinn, & Zeitlin, 2015). Heckman (2000) offers an analysis of the merits and problems in the design-based program in contrast to the previous empirical approaches in econometrics (see also Heckman, 2005). Angrist and Pischke (2015) discuss and elaborate on the details of the econometric techniques that are most commonly used in quasi-experimental studies, namely instrumental variables, regression discontinuity methods and difference-in-differences causal analysis. These methods are not entirely new, but the causal interpretation of their results in line with the potential-outcomes logic has just recently become quite popular in applied social studies and policy evaluations.

Methodological issues concerning the merits and problems of the potential outcomes framework have so far been mostly debated among social researchers. Intense discussions among prominent economists defending and challenging the approach have appeared in an issue devoted to the estimation of treatment effects in the *Journal of Economic Literature* (vol. 48, issue 2, 2010), and in a symposium (titled: 'con out of economics') in the *Journal of Economic Perspectives* (vol. 24, issue 2, 2010). Detailed critical expositions of different methodological aspects related to design-based methods of causal inference are neatly compiled in the *Handbook of Causal Analysis for Social Research*, edited by Morgan (2013), in which, again, most contributors are distinguished applied social scientists and statisticians.

Topics that can, and perhaps should, be of interest to philosophers of economics are, for instance: (a) to what extent the design-based approach, by relying on randomisation, is unbiased in relation to the researchers' methodological assumptions (e.g. Morgan & Winship, 2007); (b) how the approach deals or fails to deal with the pervasive heterogeneity present in almost every relevant dimension of the social realm (e.g. Brand & Thomas, 2013;

Claveau & Mireles-Flores, 2014; Hong & Raudenbush, 2013; Kuorikoski, 2012); (c) whether it is enough to know the strength of causal effects without knowing much or anything about the 'mechanisms' responsible for the causal connections (e.g. Marchionni, 2017; Reiss, 2007; Ruzzene, 2014; Steel, 2013; Weber, 2007); (d) how useful the research outcomes are for inferring and extrapolating policy implications, which are frequently meant to affect, not averages among populations, but specific target units or subpopulations (e.g. Cartwright & Hardie, 2012; Claveau & Mireles-Flores, 2016; Deaton, 2009; Mireles-Flores, 2017; Olsen, Orr, Bell, & Stuart, 2013; Peters, Langbein, & Roberts, 2017; Ruzzene, 2015) and (e) whether the particular way in which 'treatments' are implemented matters for the efficacy and effectiveness of the causal analysis (e.g. Favereau, 2016; LeRoy, 2016, 2018).

Evidence Diversity and Evidence Amalgamation

Epistemological issues of causal inference are directly connected to questions about the types and inferential roles of scientific evidence. Inquiries about how one can reliably acquire causal knowledge overlap with questions about what counts as good evidence for supporting causal claims and about the problems and merits of different evidential methods for testing hypotheses. Reiss (2011) provides a splendid philosophical overview of the different approaches to what evidence is ('theories of evidence'), and of the different accounts about how evidence can be said to confirm or refute causal hypotheses ('theories of confirmation').

The debate in economics between theory-based versus design-based econometrics is, in essence, a debate about which methods provide more reliable evidence for telling apart causal connections from spurious correlations (see Angrist & Pischke, 2010). In a similar vein, philosophers have wondered about the epistemic value of the different kinds of evidence that practicing scientists use to confirm or disconfirm causal hypotheses.

The methodological literature on the epistemic value of evidence to test causal hypotheses occasionally refers to slightly different things when discussing evidential diversity. Sometimes it means distinct broad kinds, such as probabilistic versus mechanistic, quantitative versus qualitative, or theoretical versus empirical evidence. Other times it means specific results obtained by using different types of evidential methods, such as econometric techniques, experimental and quasi-experimental designs, observational studies, historical studies, surveys, simulations, case studies, interviews, and common sense. The relevant question for social research is: if there are different types of evidence, which one is the best for supporting scientific causal knowledge?

According to Russo and Williamson (2007) there are at least two types of evidence commonly available to empirical scientists: probabilistic and

mechanistic evidence. Furthermore, they suggest – in what has come to be known as the 'Russo-Williamson thesis' – that to establish a causal connection, scientists jointly require probabilistic evidence and evidence for the existence of a mechanism connecting the cause and the effect. This thesis has been usually exemplified with cases from health sciences, such as the connection between smoking and lung cancer (see Gillies, 2011). In contrast, Claveau (2012) has contended that the Russo-Williamson thesis is a proper characterisation of standard economic practice, since economists often establish causal claims using only one kind of evidence. He makes this point by using some examples taken from recent studies in labour economics.

Endorsing a pluralistic stance about types of evidence, Claveau (2011) argues as well that the debate between theory-based and design-based econometricians can be better understood as a healthy stage towards accepting the epistemic benefits of diverse co-existing kinds of evidence that are available for causal induction. But what exactly is the epistemic significance of using diverse kinds of evidence to support causal hypotheses? Schickore and Coko (2013) provide an instructive general 'metaphilosophical' discussion on the potential virtues of using diverse means of determination. In the context of econometric models, Moneta and Russo (2014) elaborate on the implications of evidential pluralism and argue that it is precisely the 'integration' of different types of evidence – in their case, probabilistic and mechanistic – that allows researchers to make causal interpretations of statistical models. Shaffer (2015) is an illustration in economics of a form of pluralism ('structured pluralism') of methods of causal analysis of poverty studies. Also with a pluralistic motivation, Kuorikoski and Marchionni (2016) argue that the use of multiple and independent sources of evidence in causal investigation can be understood as a form of triangulation. Triangulation contributes to the controlling for potential errors and biases of data-generating procedures, which in turn improves the reliability of any causal analysis.

Once it is accepted that there are different kinds of evidence, and that they all have epistemic virtues, new issues arise: how can studies based on different types of evidence, which yield different or contradictory results, be compared? When highly different kinds of evidence – say, qualitative and quantitative – in different studies are being considered, how can the results be meaningfully integrated? A general evaluation of these questions has been provided by Stegenga (2013), where he proposes an impossibility theorem of evidence integration (analogous to Arrow's impossibility theorem). As a reaction, Lehtinen (2013) criticises Stegenga's analogy and argues that the epistemic problems of evidence amalgamation are not as devastating as Stegenga's analogy might lead us to believe.

Methodological problems of evidence diversity and integration have been much discussed in the context of the so-called evidence-based movement (discussed below). In the social sciences, evidence integration has also been discussed within the small academic circles of specific research programs, such as

the literature on meta-analyses (e.g. Nelson & Kennedy, 2009), meta-regressions (e.g. Stanley & Jarrell, 2005) and qualitative research methods (e.g. Major & Savin-Baden, 2011; Morse, 2006; Voils, Sandelowski, Barroso, & Hasselblad, 2008).

Evidence-based Economics and Evidence for Policy

Since the 1980s, researchers mainly in medical science and later in other disciplines have been advocating a more evidence-based approach to scientific practice. This research standpoint emerged as a reaction against previous research based either on pure theory or on what were often considered low-quality and unreliable forms of evidence, including unsystematic experience-based accounts, common sense, informal conventions, and expert opinion (see Cochrane, 1972; Guyatt et al., 1992; Petty, 2006; Sackett, Rosenberg, Gray, Haynes, & Richardson, 1996). The main idea motivating the evidence-based approach is that empirical sciences should devote more effort to improving and systematising their evidence-evaluating methods and standards. The aim is to make such methods and standards as 'scientific' as possible, with 'scientific' here meaning: conducive to high levels of accuracy combined with a minimum amount of subjective influence on the research outcomes (see Worrall, 2007).

Most philosophical discussions have focused on probing procedural difficulties related to dealing with evidence diversity, such as: How can we comparatively evaluate the epistemic weight of different types of evidence? How can evidential rankings be objective? Is the best available evidence always the best in relation to all types of scientific problems? Is all the evidence labelled 'non-scientific' dispensable? (see Ashcroft, 2004; Barton, 2000; Borgerson, 2009; Howick, 2011; Solomon, 2011; Stegenga, 2011; Worrall, 2002, 2007).

Regarding the original evidence-based movement in medical practice, philosophical criticism has disproportionately focused on the implicit and not thoroughly justified view that randomised controlled trials (RCTs) are the best available type of evidence (together with meta-analyses and systematic reviews of RCTs). As most critics have argued, it is not obvious why RCTs would necessarily have a higher epistemic weight in comparison to other types of evidential sources (e.g. Ashcroft, 2004; Borgerson, 2009; Vandenbroucke, 2004, 2008).

Debunking RCTs as the gold standard of empirical evidence has triggered renewed reflections on the virtues of alternative non-experimental types of evidence, and on the supposedly objective grounds on which evidence rankings are constructed by proponents of the evidence-based movement. For instance, a number of studies have appeared on the epistemic virtues of observational studies (e.g. Benson & Hartz, 2000; Black, 1996), expert knowledge (e.g. Collins & Evans, 2007; Martini & Boumans, 2014; Selinger & Crease, 2006), case studies

(e.g. Gerring, 2007; Ruzzene, 2012) and other forms of qualitative evidence (e.g. Silverman, 2001 [1993]).

In relation to economics, Reiss (2004) has argued specifically for a more evidence-based economic science, with the intention of supplementing what he calls a theory-based orthodoxy in current economics. Reiss's idea is that one goal of the economic methodologist should be to assess whether causal hypotheses in economics are supported by the best available evidence (of any kind), by means of using the best available appraisal techniques (to control for potential sources of error), such that they are more likely be claims about genuine causation (see Reiss, 2008).

The evidence-based movement has had an obvious influence on economics in the form of the design-based approach to empirical research, especially in development economics (see, e.g. Banerjee & Duflo, 2011; Cohen & Easterly, 2009). However, notice that RCTs are not the most common method of causal inference in economics at all, especially in policy-oriented branches of empirical economics like health economics, monetary economics, public economics, labour economics, international trade economics, economic growth and environmental economics. Econometric regressions and simulations (theory-based or otherwise) have for a long time been the preferred evidential tool in most applied branches of economics. As Swann (2006) suggests, it seems that further methodological reflection on the potential epistemic benefits of using different evidential techniques in economics is still required to properly assess whether current research outputs are indeed supported by the best available evidence.

A widespread idea somewhat connected to the evidence-based approach to casual analysis is that improving the evidential standards of science by using the best evidential methods will result in scientific knowledge that is more reliable to support practical and policy applications. In the last decades, this idea has been criticised by philosophers on the grounds that it ignores the several difficulties of extrapolating policy recommendations from well-established scientific studies to different actual target situations. In particular, Nancy Cartwright – together with a number of collaborators – has produced a substantive amount of research exposing and urging for solutions to the 'external validity' issue with scientific results.

Cartwright (1979) was one of the main proponents of the probabilistic account of causality (together with Skyrms, 1980; Suppes, 1970; Eells, 1991). More recently she has defended an account of capacities (see Cartwright, 1989, 1998) as the relevant and invariant causal powers responsible for actual manifestations of causal effects. Most of her writings on causality during the last two decades have focused on criticising univocal accounts (see Cartwright, 2007), and some of the modern accounts of causal inference as well, such as Bayesian nets methods (e.g. Cartwright, 2001) and accounts based on modularity (e.g. Cartwright, 2002). Cartwright's current position can be taken as a form of pluralism about the meaning of causation (see Cartwright, 2004). More recently, she has written on the use of scientific evidence for policy purposes or

what she labels 'evidence-based policy' (see, e.g. Cartwright, 2009; Cartwright & Hardie, 2012; Cartwright & Stegenga, 2011).

In Cartwright's view, the evidence-based movement fails to provide a good basis for effective policy by wrongly placing a great deal of trust on particular evidential methods, such as randomised controlled trials (see Cartwright, 2009, 2010). The evidence-based movement claims that RCTs are good evidence to support claims about implementing 'T in order to bring about outcome O', but Cartwright argues that RCTs only support 'claims of one particular form, essentially, "T causes O in particular circumstances X in particular population Φ"' (2009, p. 129). But if one wants to bring about the outcome O in a completely different population, how can one be sure that T will be effective there as well?

Cartwright and many others often refer to this issue as the external validity problem: 'for what other populations can we expect these same conclusions to hold?' (2006b, p. 986). Is it possible to extrapolate a valid causal result obtained in a particular setting to a different setting and expect the same result to obtain (see also Bareinboim & Pearl, 2013; Grüne-Yanoff, 2016; Olsen et al., 2013; Peters et al., 2017; Ruzzene, 2015; Steel, 2010)? The possibility of extrapolating causal results for policy purposes is of course a concern that affects most, if not all, empirical methods of causal inference (see Mireles-Flores, 2016).

What are, then, the evidential requirements that a well-ordered science should follow in order to achieve reliable extrapolation of policy results? Cartwright's main aim in her most recent contributions has been to develop an account about how to deal with 'external validity' for policy purposes by taking into consideration all the contextual causal conditions (enabling and disturbing factors) that are relevant for the causal effect to obtain (see Cartwright & Efstathiou, 2011). The most recent outcome of this project can be found in her book with Hardie (Cartwright & Hardie, 2012). According to Marcellesi (2015), Cartwright and Hardie have in fact succeeded to a great extent in dealing with the problem of external validity, by characterising the conditions under which it is allowed to predict, from the truth of a causal claim in one situation, its truth in a different situation. It remains to be seen whether this type of account can be implemented (and to which extent) in actual practice.

BEHAVIOURAL ECONOMICS

Behavioural economics is an approach developed during the 1980s by academics who judged the psychological assumptions of mainstream economics as unacceptably unrealistic. The disputed content were the core assumptions of rational choice theory, which constituted the central foundations of mainstream economic theory at the time. The emergence of this approach was instigated by a 'cognitive revolution' in psychology, whereby researchers challenged the

traditional 'behaviourist' stance, and began developing computational models of individuals' mental representations and learning processes (see Nagatsu, 2015a). Motivated by the advances in psychology, researchers in microeconomics started taking seriously the emerging empirical evidence undermining standard assumptions about human rationality, and subsequently tried replacing such assumptions with the most recent empirically grounded findings from experimental cognitive sciences.

The proponents of the behavioural approach held that making more realistic 'the psychological underpinnings of economic analysis will improve the field […] generating theoretical insights, making better predictions […], and suggesting better policy' (Camerer & Loewenstein, 2004, p. 3). Consequently, most methodological inquiries about behavioural economics have focused on whether the approach has lived up to its epistemic expectations in terms of yielding better explanations, predictions and interventions.

Angner and Loewenstein (2012) describe three phases in the development of behavioural economics: first, a period of acceptance and assimilation of the surmounting empirical results which identified anomalies and inconsistencies in standard rationality theory, such as preference reversals, loss-aversion, framing effects, hyperbolic discounting, and anchoring effects (e.g. Thaler, 1981; Tversky & Kahneman, 1974, 1981). Second, a period of development of novel economic theory upon the basis of new empirically grounded assumptions about rationality and about individual human behaviour (e.g. Akerlof, 2002; Kahneman & Tversky, 1979; Rabin, 1993; Starmer, 2000; Thaler, 1980, 1985). And third, a still ongoing phase characterised by attempts to apply insights from behavioural economics to public policy (e.g. Diamond & Vartiainen, 2007; Thaler & Sunstein, 2003). For some comprehensive accounts of the history and development of behavioural economics, see the edited volume by Camerer, Loewenstein, and Rabin (2004) or Angner's (2012) introduction to behavioural economics. Heukelom (2014) has written an ambitious book describing in detail the different historical stages of behavioural economics. In a much more concise piece, Nagatsu (2015a) reviews the distinct methodological waves of the approach and describes some of the main criticisms to its most recent developments.

A number of methodologically rich studies on the history and implications of the 'cognitive' approach to economics have appeared in recent years. For instance, Esther-Mirjam Sent's (2004) article on the interactions between psychology and economics is a historical piece with a strong focus on the progress (and twists) in the genealogy of behavioural economics. Bruni and Sugden's (2007) text is an appraisal of Pareto's influence on the separation of scientific economics from psychological presuppositions in modern neoclassical theory. In an article motivated by Mark Blaug's methodological ideas, Dow (2013) evaluates whether the 'new' behavioural economics research program is 'progressive' or 'degenerative' (in Popperian/Lakatosian terms) in relation to mainstream economics. Małecka and Nagatsu (2018) aim at characterising the

influence of behavioural economics on consumer law scholarship, and to do so, they start by offering an illuminating description of the evolving stages of the research, first in psychology, and then, in economics.

The attempt to provide economic theory with more solid empirical foundations involved discussions about the validity of alternative methods to those of standard theoretical neoclassical economics, such as thought and laboratory experiments, field and natural experiments, and computer simulations (see Angner & Loewenstein, 2012, pp. 668–675; Boumans, 2016; Guala & Mittone, 2005). In a review of the already mentioned volume *Advances in Behavioural Economics* (Camerer et al., 2004), Fudenberg (2006) offers an early critical assessment of the whole approach and points out a number of problematic aspects related to generalizing the validity of its results.

In recent years, the methodological discussions in relation to behavioural economics have focused mainly on four broad topics: (a) the viability and epistemic merits of neuroeconomics, (b) whether behavioural economics is truly an alternative to neoclassical economics or only a more sophisticated variation of it, (c) the appraisal of the normative and ethical implications of the behavioural approach to welfare economics and to (d) public policy in the form of so-called libertarian paternalism.

Neuroeconomics

The main idea inspiring neuroeconomics is to use the most recent neuroscientific evidence and tools to study the neural foundations of economic choice behaviour. The project of combining neuroscience with economics has motivated questions about the feasibility, epistemic virtues and consequences of methodological interdisciplinarity. Most contributions have focused on questions such as: How are the methods in economics affected or enriched by neuroscience? Is there any real improvement in economic knowledge and understanding after the introduction of neuroscientific results in economics? What is the real impact of neuroeconomics in relation to traditional methodological issues in economics, for example, realisticness of models, explanatory relevance, and predictive power of economic science?

Some noteworthy methodological contributions are the following. Harrison (2008) offers an appraisal of the potential of neuroeconomics, and argues that – at least up to 2008 – the approach was marketed and sold as achieving much more than was really the case. Mäki (2010) exposes and comments on the rhetorical strategies used in neuroeconomics. Vromen (2010) reflects on how unsurprising some 'surprising' neuroeconomics findings are. Ross (2012) offers a neat overall description of the project, while being rather critical of its achievements. Fumagalli (2013) elucidates on how neuroeconomics *has tried* to inform theory on the foundations of welfare economics. And Clarke (2014)

evaluates arguments in favour of and against neuroeconomics in relation to distinct interpretations of what the aims of economics are.

In the last decade, there have been at least three special issues devoted to philosophical and methodological appraisals of neuroeconomics: an issue in *Economics and Philosophy* (2008, vol. 24, issue 3), another in the *Journal of Economic Methodology* (2010, vol. 17, issue 2) and one more in *Biology and Philosophy* (2011, vol. 26, issue 5). The majority of the methodological assessments of neuroeconomics have tended to be rather negative, at least, in relation to the relevance of neuroscientific studies to the progress in economics: 'more hype than substance' seems to be the consensus (see Marchionni & Vromen, 2012). Still, there are authors who have focused on some positive features as well. For instance, Herrmann-Pillath (2016) uses the case of neuroeconomics to argue in favour of developing constitutive explanations (which in turn account for the role of mechanisms) in the integration of economics and the neurosciences.

A clear and insightful article on the current methodological debates in neuroeconomics is Fumagalli's (2016b) 'Five theses on neuroeconomics'. He formulates questions and provides answers to the following controversial issues: (1) Does neuroeconomics achieve unification? (2) Is the standard economic theory improved thanks to neuroscientific results? (3) Do neuroeconomics' findings advance model selection in economics? (4) Which other disciplines enrich neuroeconomics' accounts of choice? (5) Does neuroeconomics amount to an expansion of the evidential base of economic theory?

Is Behavioural Economics Different from Neoclassical Economics?

As some commentators have suggested, the success of behavioural economics is to a great extent a consequence of an innovative scientific interdisciplinary exchange plus a good rhetoric (e.g. Angner, 2014; Nagatsu, 2015a). Behavioural economists have indeed accomplished the introduction of exciting empirical insights from cognitive sciences into economics, while at the same time adopting the basic concepts, language and modelling techniques of mainstream economics to present their results, rather than radically departing from the prevailing neoclassical theoretical attitude. After several Nobel prizes awarded to the pioneers of the approach and the increasing endorsement of behavioural public policies, some authors understandably wonder: But how revolutionary is behavioural economics really?

Rubinstein (2003) has questioned the innovativeness of behavioural economics, particularly in the treatment of hyperbolic discounting. Basically, Rubinstein fails to see the concrete psychological input in the revised models. A proper attempt at combining 'economics and psychology', he argues, would require opening the black box of the individual decision makers, instead of

simply modifying and developing more sophisticated mathematical formal accounts, totally in line with the neoclassical style.

More recently, Berg and Gigerenzer (2010) have claimed that all branches of behavioural economics are in spirit just neoclassical economics in disguise. Their point is that the new revised accounts to characterise human behaviour do not refer to any proper empirically grounded psychological processes in order to explain the data under investigation. Instead, the new approaches mainly rely on *as-if* justifications for their psychological presuppositions, which in Berg and Gigerenzer's view renders them essentially indistinguishable from the standard neoclassical accounts of which they were supposed to be an improvement.

Ross (2014a, 2014b) makes a similar criticism, but with a different emphasis. According to him, behavioural economics might wind up being just a branch of psychology, since it does not really engage with proper economic phenomena. Broadly put, the criticism is that using insights from psychology – such as accounts of the psychological dispositions of individuals – to characterise utility functions for representative agents precludes researchers from answering the questions of real interests in economics, specifically: How to model and estimate the structure of aggregate heterogeneous behaviour from available macroeconomic data sets? Most economists, according to Ross, are mainly concerned with, say, aggregate consumer behaviour (perhaps taking into account different representative consumer types), while the psychological dynamics going on in the mind of any particular consumer are only of secondary relevance.

As Nagatsu (2015a) argues, behavioural economists currently lack acceptable responses to all these criticisms, namely: to those who question the substance of the psychological inputs into economic theorising, to those who question the usefulness of the project to account for relevant economic phenomena, and to the general question of how exactly behavioural economics differs from (or improves upon) standard neoclassical modelling practices in economics.

Behavioural Welfare Economics

One reason for the rapidly growing popularity of behavioural economics is that it allegedly has some obvious and direct normative and policy implications of public interest. Understanding the psychology of choice-makers in a more realistic manner is said to be useful for designing more effective and efficient social policies and institutions. This has been the case, for instance, in relation to inter-temporal inconsistencies in saving behaviour, about individuals' perceptions of financial risk, or about labour behaviour and how workers perceive inflation and different risks (see Kopcke, Little, & Tootell, 2003; Laibson et al., 1998).

The normative implications of behavioural economics have been predominantly discussed in connection to what is sometimes called behavioural welfare

economics. Broadly put, the systematic anomalies in relation to the standard assumptions of instrumental rationality which have been found since the 1980s (i.e. preference reversals, loss-aversion, framing effects, hyperbolic discounting, and anchoring effects) have cast doubt not only on standard rational choice and microeconomic theory, but also on the 'standard methods of normative analysis' traditionally employed in welfare economics. According to McQuillin and Sugden (2012), the fundamental theorems of welfare economics are based on the typical assumptions about coherent individual preferences in line with instrumental rationality. Therefore, the behavioural 'anomalies' that represent a challenge to rationality, are also a challenge to the standards by which applied welfare economics determines which goals are socially desirable. Thus, as McQuillin and Sugden suggest, there is an indirect tension between the results of recent empirical research on behavioural economics and the outcomes of applied welfare economics (or any normative method of policy evaluation based on welfare economics).

The methodological discussion related to the purported problems of normative economics has focused either on the ontology of preferences and other mental states which are said to determine economic behaviour (e.g. Angner, 2018; Dietrich & List, 2016; Guala, 2012, 2017; Hands, 2012; Hausman, 2012; Infante, Lecouteux, & Sugden, 2016) or on the normative justifications of public policy standards of what social 'welfare' is assumed to involve, such as, wellbeing (e.g. Kahneman & Sugden, 2005), liberty (e.g. Sugden, 2007, 2008), autonomy (e.g. Hausman & Welch, 2010, Mills, 2015, Nagatsu, 2015b, Pinto-Prades & Abellan-Perpiñan, 2012).

A much less discussed issue of methodological interest is the shift that theories of rationality underwent from a descriptive to a normative status after the initial behavioural anomalies emerged (see Hands, 2015b; Herfeld, 2017). Recently, Małecka (2017) has questioned the very idea that a theory can simultaneously be intended to be explanatorily adequate and normative. Yet behavioural economics, she argues, seems to be proposing a better descriptive account of behaviour, while at the same time conceding on the use of expected utility theory for normative purposes.

Libertarian Paternalism

A rather noticeable consequence of the cognitive movement in economics has been its impact on actual public policy debates, especially in relation to what has been called *libertarian paternalism*, or 'nudging'. Moreover, it is clear that books like *Nudge* (Thaler & Sunstein, 2008) and *Animal Spirits* (Akerlof & Shiller, 2009) have contributed to the widespread popularity of behavioural economics and their policy potential. As a consequence, academic and non-

academic discussions about nudging policies and their philosophical and ethical implications have proliferated.

Libertarian paternalism is the use of policy interventions to *subtly* influence people's choices, such that they end up choosing options that would make them better off. The intervention takes place without imposing any restrictions directly on the individuals' choice behaviour, but rather by intervening on the context or environment of choice, called 'choice architectures'. Nudging is based on the overwhelming amount of empirical evidence about failures and anomalies of traditional theories of rationality. Different choice architectures are expected to influence individual behaviour by prompting the biases and heuristics that individual agents commonly employ in real world choice situations, allegedly without inflicting any restriction on the individuals' freedom to choose among available alternatives. Osman (2016) provides a clear overall discussion of the general aims, problems and defences of the approach.

Not surprisingly, ethical questions with regard to this approach have ignited philosophical debate. Behavioural findings have shown that individuals often fail to make the best choices for their own benefit. Nudge policies are meant to remedy that 'problematic' situation, and thus in principle help people be better off according to their own preferences. But, how can the policy maker know what the best is for people? (e.g. Anderson, 2010; Bovens, 2009). More specifically, how can the nudge architect be sure to properly account for what individual choice-makers actually prefer (e.g. Guala & Mittone, 2015)?

Is nudging really respectful of individual liberty? How much libertarian paternalism actually intrudes or restricts the liberty of choice-makers by employing a form of subtle manipulation (e.g. Hausman & Welch, 2010; Nagatsu, 2015b; Wilkinson, 2013)? Would it not be better, instead of nudging individuals, to boost them via 'rational persuasion' (e.g. Hausman & Welch, 2010)? Are nudge policies intruding on the autonomy of individuals? How much of the policy maker's will is imposed on the individuals (e.g. Grüne-Yanoff, 2012; Reiss, 2013, Chapter 15)? In a clarifying assessment of most of the debate, Heilmann (2014) provides a systematic characterisation and typology for nudges. Moreover, he proposes, first, a set of *conceptual* conditions that are necessary and sufficient for nudges to be successful, and second, further *practical* conditions for successful implementation, transparency and justification of nudging policies.

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